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Edited by
Margaret Macdonald
with the advice of
A. J. Ayer — A. E. Duncan-Jones
R. B. Braithwaite — C. A. Mace
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H. H. Price

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ANALYSIS 15 : 4 March, 1955

HOW *NOT* TO TALK

(*Pedantic Objections to Professor Austin*)

By D. R. COUSIN

IN Professor J. L. Austin's paper " How to Talk ", read before the Aristotelian Society, June 12th 1953, he tacitly departs in certain respects from the way of speaking which he adopted in his contribution to the Symposium on *Truth* printed in the Aristotelian Society's Supplementary Volume XXIV, e.g. on p. 116. About these innovations, he says : " to talk of types and senses, and of matching the one to the other, is not necessarily inexpedient in all contexts " (p. 230). I don't disagree. But it may be worth while to point out some of the ways in which it *might* be inexpedient in some contexts. He professes himself " engaged to elucidate some of our ordinary language about speech-acts " (p. 230). But I think that, even if certain of our speech-acts do conform to a model like the one which he discusses, the language in which he discusses it exhibits certain inconsistencies which need not be attributed to our ordinary language about such speech-acts and is likely to lead rather to obfuscation. I do not wish to call in question the substance of his discussion of " placing ", " casting ", " stating " and " instancing ", nor to suggest that Austin is unaware of the features of his metalanguage to which I call attention.

I

Austin considers only sentences of the form " I is a T ". If actual sentences are to be constructed we require a number of I-words and T-words to fill the gaps on either side of the formula " — is a — ". And if we are to understand what is said we shall have to know what non-linguistic entities these words respectively stand for (if so crude an expression may be permitted). This is brought about by what Austin calls semantical legislation. (It seems to me that the analogy is closer with the activities of the Registrar at a civil marriage, which are I suppose administrative rather than legislative.) As a result of this legislation, the language (S_0) comes to possess two classes of vocables, " I-words " or " references " (Austin uses numerals, e.g. " 1227 "), semantically correlated with (non-linguistic) *items* ; and " T-

words" or "names", semantically correlated with (non-linguistic) *types*. The non-linguistic entities, items and types, are assumed to be so related to one another that every item is *of* one (and for his purposes only one) type whereas each type is of at least one item (possibly more). Sometimes (I suppose when he wants to emphasise the ubiquity and intimacy of these non-linguistic relations) Austin replaces "item" by "item/type" and "type" by "item-type". Very roughly, "item" (in Austin's metalanguage) is a translation of "individual thing" (in English); and "type" (in Austin's metalanguage) is a translation of "kind of thing" or "characteristic" (in English).

Austin does not further concern himself with the nature or genesis of I-conventions, and in particular he has no word in his metalanguage for the legislative (or administrative) act by which particular I-conventions are set up. In English, I suppose we should call it *name-giving*. But in Austin's metalanguage "name" = "T-word", which might be roughly translated into English by "common noun". Hence for him "name-giving" has a different use, as we shall see at once.

"T-conventions, or conventions of *sense*, are needed in order to associate the vocables which are to be T-words with the item-types, one to one: these conventions we may inaugurate by one or other of two procedures of linguistic legislation, viz.

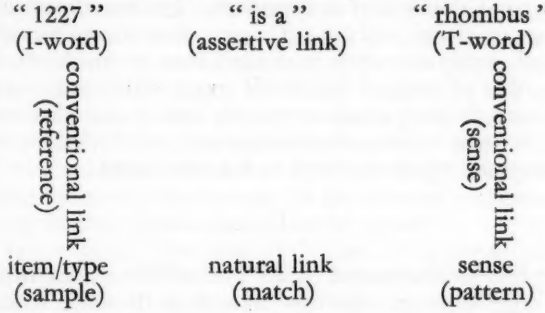
1. Name-giving.
2. Sense-giving.

Name-giving ("naming" in one ordinary sense, but not, e.g., in the sense of "giving the name of" or of "putting a name to") consists in allotting a certain vocable to a certain item-type as its "name". Sense-giving ("defining", in the sense of "ostensive definition", here in a simplified world) consists in allotting a certain item-type to a certain vocable as its "sense". These two procedures, at least in our simplified situation, produce the same upshot: when either has been gone through, the item-type, attached by nature to certain items, is attached by convention to a certain vocable, now a T-word and (as we shall call it) its "name", as the "sense" of that word" (p. 229).

On the face of it, Austin treats this procedure as if it were closely analogous to the assigning of numerals to items, but quite distinct from it. Items would not be involved here at all. So far as this version goes, types would be conceived (quasi-Platonically) as individual entities (of course of a different kind from items), each of which would be called up to the platform and married to its name. The two would retain their separate

identities ; but after the marriage the noise " rhombus ", say, would be the *name* of a certain shape (type) and that shape would be the *sense* of that noise (name). In English, a man and a woman, so called to signalize their respective anatomical and physiological characteristics, may be called in addition *husband* and *wife* to signalize a conventional relation to one another. Similarly, " name " and " sense ", in Austin's metalanguage, are applied to things which already have descriptions referring to their nature as noises or shapes, in order to signalize their semantical relationship to one another. Thus a type and a sense are not two different things, any more than a woman and a wife, but simply the same thing differently described. There may, indeed, be types which are not senses (because no name has been given them) just as there are women who are not wives. But there are no wives who are not women, nor senses which are not types. Nor are a noise and a name two different things, but the same thing considered first without and then with respect to its semantical employment.

Leaving aside for the moment the very different account of name-giving which appears on p. 230, let us consider a certain inexpediency which arises from this account of name-giving. When we come to use our language to talk about the world, a satisfactory utterance " will be one where the item referred to by the I-word in accordance with the conventions of reference is of a (in S_o, the) type which matches the sense which is attached by the conventions of sense to the T-word. For the utterance to be satisfactory, we require the presence of *both* a *conventional* link between I-word and item, and another between T-word and sense, and a natural link (match) between type and sense " (p. 230). The situation is illustrated by a diagram (p. 231) :—



Ignoring for the moment the appearance in the diagram of

the terms " sample " and " pattern ", and accepting the representation of the non-linguistic features of the situation by the expression " item/type " in the bottom left-hand corner, let us inquire what we are to make of this alleged " match " between type and sense. If Austin's metalanguage were ordinary English, " match " would be a 2-place predicate taking individuals as its arguments. They would necessarily (as a matter of logic) be different individuals, though if they were correctly said to match they would *resemble* one another in some respect, or in all relevant respects. Thus we might well expect to be told that Item 1227 matches Item 1228, if and only if they are items of the same type. But it would be nonsense to say that an item matches a type or that two types match one another. (For, from the nature of a type as a kind of thing or a characteristic there is no third alternative between identity and difference.)

Now, in Austin's account, as we are reading it at present, no items are involved in the match to which he refers. The only item mentioned comes in merely to identify a certain type, and it is this *type* which is said to match the sense of a certain name. But this must be nonsense. The sense of the name in question, as we have seen, is a certain type. And this type either is different from the one it is said to match, as square is from rhombus, or it is identical with it. In the first case the utterance will not be satisfactory ; in the second, there will not be the sort of numerical difference which is presupposed by matching, in the ordinary sense.

Of course, Austin may say : "Ah, but *my* use of " match " is different. What I mean when I say that A matches B is that "A" and " B " are two different names for the same thing. Matching, for me, is the semantical form of identity." This may be true, but I doubt if it is expedient. For the use of a word which has a quite different kind of logic in English is almost bound to mislead, especially when, as I shall now try to show, Austin gives another account of the whole thing which, up to a point, fits ordinary English usage admirably but is quite inconsistent with the account of semantical legislation, and with those aspects of the diagram, which we have so far considered.

II

Here is Austin's defence (p. 230) of his way of describing the (simplified) language situation against a possible charge of inexpediency. " Conceive of our items here as, say, a number of

samples or *specimens* of colours, or of (geometrical) shapes, each with a reference-numeral allotted to it: conceive of our senses as a number of standards or *patterns* of colours, or of (geometrical) shapes, each with a name allotted to it: think of name-giving or sense-giving as involving *the selection of a sample or specimen as a standard pattern*. This is not so far from the truth."

Notice that here there is on the face of it no reference at all to types as (quasi-Platonic) individuals (as it might be of a different 'ontological status' from items) which are to be married to T-words (names). Indeed, unless we are to understand it as implicit in the use of "sense", "pattern" and "standard", there is no reference to types at all.

Let us examine this account on its merits. To begin with, let us dismiss from our minds what we have so far been led to believe about Austin's use of the word "sense", and see what we can gather about his intended use of it from what he says in the present passage, together with the introduction (which we must now take into account) of the terms "sample" and "pattern" in the diagram.

As before, I find nothing remarkable in what is said about items and their semantical spouses the reference-numerals. What strikes me as specially noteworthy is the passage italicised in the account of name-giving or sense-giving: "*the selection of a sample or specimen as a standard pattern*". For in ordinary English, it seems to me, every one of these words is normally applied to what Austin would call *items*, and never to what he seems to mean by "type". Of course to call an item a *specimen*, *sample*, *standard*, *pattern* (or *standard pattern*, if that surely redundant expression is useful at all) is appropriate only because of certain relationships, or, if we please, because of the type which it is of. Roughly, an item is called a *sample* or *specimen* because there are other items of the same type, which it is *taken* in some way to represent; and it is *made* a standard or pattern (in this context—of course there are others) by being married to a T-word or name. This name thus comes, according to this version of Austin's theory, to be the name *of the standard item*. The point I wish to emphasize here is that in this passage Austin seems to be saying (correctly) that names (in his sense of common nouns) are given to *items* (individuals), not to types.

So far, so good. But what of "sense"? In the act of semantical legislation a T-word or name is married to what thereby becomes a *sense*. It seems fair to suppose that here too the analogy of "wife" holds, i.e. that "sense" is to be a word (in

the metalanguage) applied to something to mark some semantical relationship, i.e. the fact that it has a name. In this context, accordingly, " sense " too should apply to *items* not to types, since it is to items that names are given. *Which* item shall we call the *sense* of a particular I-word, say of " rhombus "? We are to " conceive of our senses as a number of standards or *patterns* of . . . shapes, each with a name allotted to it " (p. 230), which seems sufficiently unambigious: participation in the ceremony of semantical marriage may be thought to confer some special claim to the title ' sense of the T-word " rhombus " ' upon the item which has already, by the same token, been created *standard* or *pattern*. Moreover, if we inquire how the terminology of the passage at present under discussion fits into the diagram of p. 231, it seems that a good fit is obtained if we take the sense of the name " rhombus " to be the standard item or pattern. For, since we are now no longer considering types as entities of a different kind from items, but are thinking only of items themselves, we can give the normal interpretation to " match " in the bottom line of the diagram. It will be a two-place predicate the arguments of which are two different items, in this case of the same type.

The general semantical theory which Austin would then be expounding would not be entirely devoid of plausibility: we *could* represent the utterance of such a sentence as " 1227 is a rhombus " as expressing a comparison of 1227 with (say) 1007, the item semantically married at some time in the past to the T-word " rhombus ", or as asserting a resemblance between 1227 and 1007. But I do not think that this theory would do, nor that it is Austin's theory. (He rejects it in his contribution to *Truth*, p. 116, n. 9).

We do, I believe, learn the use of common nouns in the course of applying them to items. (What else could we apply them to?) But when we call some newly-met item a rhombus we are NOT thinking of some other specific item through which we were introduced to the name " rhombus ". And, while it seems to me in accordance with our ordinary language about speech-acts to say that a name is the name of an item, it does not seem equally plausible to say that a common noun is the name of some *particular* item, *viz.*, the standard or pattern. It seems more plausible to say that when a sample item is made the standard or pattern by being married to a noise, it is so married as proxy for other items; and that the noise thereby comes to be the name of the standard item *and of any other items of the same*

type. (In a metalanguage which uses the word " denote ", we may say that it denotes these items ambiguously.) The main, if not the whole, point of the semantical ceremony is to have ready a name by which to call other items of the same type, should any appear. Names (in Austin's sense of common nouns) are indeed given to items ; not like references, one vocable to one item (to mark their distinctness), but on the contrary one vocable to many items (to mark their similarity).

What then becomes of our suggestion that the *sense* of a name is the item of which it is the name? No one item can be so called, any more than any one woman is *the* wife of a polygamous husband. But we certainly do not want to say that a word which names a number of items of the same type has as many *senses* as there are items. This would suggest the ambiguity of the English word " box ", or a relation of a name to a plurality of *types* ; a thing which Austin's semantical morality would stigmatize as most bestially sinful.

Moreover if we suppose Austin to mean that *any* item of suitable type is a (not the) sense of the word " rhombus " then in the satisfactory utterance illustrated by the diagram the item/type of sample of which " 1227 " is the reference will already *be* a sense of the word " rhombus ", and will be (logically) incapable of *matching* the sense of the word " rhombus " except in Austin's sense of (semantical) identity.

If the above is correct, Austin's metalanguage is misleading in both of the two interpretations we have considered. Can we find the cause and suggest a remedy? It seems to me that Austin has allowed himself to be misled (or has not taken sufficient care to avoid misleading us) by a supposed analogy between naming, in his sense of introducing a common noun, and the naming of an item or individual. Perhaps he is also affected by the belief (superstitious, as we have been shown by Wittgenstein, Ryle and others) that if a word has a sense there must be some thing (item or type) which it has, i.e. to which it has acquired a conventional relation. In what follows I shall try to show how Austin's metalanguage should be modified in order to bring it more into accord with the logic of the English words which he perverts.

III

Let us return to the account on p. 230. Name-giving or sense-giving, we are told, involves " the selection of a sample

or specimen as a standard pattern". Let us assume, as was suggested above, that Austin's terms "sample", "specimen" and "standard pattern" apply to *items*. Is it in accordance with the ordinary usage of "name" to suppose that the process just described results in a word's becoming the name, not of this item, nor of any other item however similar, but of something which is not an item at all? Would not such an assumption make hay of the logic of "— is a —"? For consider: if "1227" is a proper name it makes sense (in English, though not in S_o) to point to a certain item and say "*This* is 1227". It also makes sense, if "rhombus" is a common noun, to say "*This* is a rhombus". To what, in this case, do we point? Not, of course, to *the* item of which "rhombus" is the name. For, since "rhombus" is a common noun, there is no *one* item of which it is the name. Nor, equally of course, to the *type* of which "rhombus" is the name. For we should not know how to point to a type, and the type is *not* a rhombus. We point, in accordance with the ordinary logic of "name", to an *item—any* item of the appropriate type. It seems better to say that a word which is a common noun is the name of *all* items of a certain type and not of the type which they have in common; just as a polygamous husband is the husband of all his wives and not of their femininity.

We have still to account for "sense-giving". Can we say that "sense" is the relative converse of "name"? In that case it will apply to those items of which "rhombus", say, is the name. But how? Shall we say that "rhombus" has as many senses as there are items which are rhombuses? This, as we have seen, would be most misleading. We certainly want a name to have one and only one sense *in which* it applies to a plurality of items, and we can easily see why Austin wishes to have one entity, the type, to be the sense of a (common) name.

But there is no need for this quasi-Platonic hypostatization of the univocality of common nouns. The logic of "the sense of T" is more like that of "the marital status of H" than like that of "the wife of H". We can speak of the sense of a common noun, just as we can speak of the marital status of a polygamous husband. But we cannot speak of "the wife of H" if H has taken up all his four options, though he could be described as married to W, X, Y, and Z. Not to proceed too far with the analogy, let us say that if "rhombus" has entered, with due ceremonial, upon the state of semantical polygamy appropriate to a common noun, then it *has a sense*. But by saying this we are simply saying that it is semantically correct to apply this

name to certain items but not to others. And if we are asked *what* is the sense of "T" we must reply by pointing to a specimen, preferably perhaps the standard or pattern, saying not "This is it", but "Here is one". In doing so, we should not be *pointing to* the sense, but we should be *showing* it. The sense, that is to say, of a T-word or common noun is neither any of the items to which it is correctly applied, not yet the type to which they belong. It is rather the semantical convention in accordance with which it is applied. Hence, if we are asked to describe a satisfactory utterance of this very simple sort, we should say that it is one in which the T-word is applied, in accordance with its sense, to the item to which the I-word refers. If we want to be able to say clearly what Austin says misleadingly in the sentence "The item/type *matches* the sense of the name", it would be better to talk of *suitability* (or *conformity* or *accordance*), and to say, e.g. that the item/type *suits* the sense of the name.

University of Sheffield.

MR. HARE AND NATURALISM

By BRICE NOEL FLEMING

IN chapter five of *The Language of Morals*, Mr. Hare tries, by giving us a new version of Moore's argument, to show plainly and finally why "naturalism" is untenable. The gist of Mr. Hare's version of the argument is found in the following words: "Let us generalize. If 'P is a good picture' is held to mean the same as 'P is a picture and P is C', then it will become impossible to commend pictures for being C; it will be possible only to say that they are C . . Whatever defining characteristics we choose, this objection arises, that we can no longer commend an object for possessing these characteristics."[1] I want to make three comments on this argument.

1. Mr. Hare says that with the naturalistic definition of 'good' it will become impossible to commend pictures for being C; he seems to mean that it will, for instance, become impossible to say "Pictures which are C are good pictures", or "Pictures are good because they are C". But this is a misleading way of putting the matter, although Mr. Hare puts it this way again and again. Most of us would agree that there are two kinds of impossibility: logical, as when we say "It is impossible for a circle to be square"; and physical, as when we say "It is impossible for a man to run a mile in one minute". Now it is logically impossible for a circle to be square; that is, we do not correctly say "This is a square circle", no matter what "this" happens to be; but it is not logically impossible for me to say "This is a square circle", no matter, again, what "this" happens to be; and it is not physically impossible for me to say "This is a square circle", because I can in fact utter the words. Presumably Mr. Hare never supposes otherwise Yet a tautology is not as bad as a self-contradiction, and a tautology is the worst that the naturalist would be guilty of if he said "P is good because it is pleasant", while holding that "P is good" and "P is pleasant" mean the same.

We all make use of tautologies, and not only in defining our terms. A teacher may say to a temporarily muddled pupil,

[1] R. M. Hare, *The Language of Morals*, Oxford, at the Clarendon Press, 1952, p. 85.

" This is a square because it's an equal-sided rectangle ", trying to get the pupil to apply something he already knows perfectly well ; so a hedonist may say to a permanently stubborn opponent, " This is good because it's pleasant ", trying to get the opponent to apply something he must know—the hedonist is convinced—but seems perversely to deny. Examples of the use of tautologies to break muddles are easy to think of ; we may also employ tautologies to make clear which of a word's meanings we have in mind, where the word has several related but different uses in ordinary talk. It should be remembered that the naturalist need not, any more than the non-naturalist, insist that a word like ' good ' has only one meaning or use, although he must, like the non-naturalist, give reasons for saying that some particular meaning is primary. This question of " primary meaning " will come up briefly again ; here I want simply to point out that the naturalist does not have to be committing a mortal sin in saying " P is good because it is pleasant ", while holding that " P is good " means the same as " P is pleasant ". For such a naturalist to commend pleasant things by calling them good might be ineffective, but it would hardly be impossible ; I do not see that it would even be improper.

2. Grant, all the same, that we do not allow ourselves, or should not allow ourselves, to use tautologies except in giving definitions. Still, we can commend things which have the defining characteristics of ' good ', and commend them for having these characteristics. If I am using " P is good " to mean the same as " P is pleasant ", I can commend pleasant P's by saying " I commend these P's because they are pleasant ". If commending is a matter of the force of the words, then these words surely have that force ; and if commending is, at least in part, a matter of effect on the hearer, then whether these words commend is an empirical question, as is the question whether " P is good " commends, whatever the meaning of ' good '. Many groups of words have, in certain contexts, the function of commending, and no naturalistic definition of ' good ' is going to keep us from commending things with logical propriety. Mr. Hare says at least once, not that a definition of ' good ' as, for instance, ' pleasant ', makes it impossible to commend pleasant things, but that such a definition makes it impossible to commend pleasant things by calling them good ; and this is the most that he has any right to say.

Even so, the hedonist will believe that pleasant things, by and large, require commending no more than good things, and

he will say that we do in fact commend things by calling them pleasant, as we do by calling them good. Further, he may argue that if " P is good " has roughly the force of " I commend P ", we ought to say " I commend P " rather than " P is good ", because the latter tends to be misleading ; the man in the street, told that something is good, hardly thinks he is merely or mainly being told that the speaker or anybody else commends it. We seem to prefer to commend things by calling them good, rather than by saying we commend or recommend or approve of them, and the reason may be that ' good ' has primarily descriptive meaning, that it refers in its most important use to what people want for themselves and for its own sake—namely, pleasure, according to the hedonist : ' good ' is better designed to commend, just for this reason. And because ' good ' has such descriptive meaning, it may sometimes be used, if misleadingly, to commend, and to do nothing else.

3. There is still the objection that we do in fact commend pleasant things by calling them good, and the hedonist's proposed definition would keep us from doing this ; hence the definition is wrong in the sense that it would keep us from saying what we now say and want to say. But Mr. Hare's proposed analysis of the use of ' good ' would keep us, in the same manner, from saying something which we do in fact say. It would keep us from saying " I commend P because it is good " ; we could no longer answer, to the question why we commend anything, that what we commend is good ; and it seems to me that we want to give this answer quite as often as we want to say something like " Pictures which are pleasant are good pictures ", or " This is good because it is pleasant ". Notice too that Mr. Hare's analysis does not affect only the relation between " P is good " and " I commend P ". In a certain situation, my saying " I'd like you to look at P " may have the function of commending P to you ; and if you ask me for a reason, I cannot say " Because P is a very good picture ", for, on Mr. Hare's analysis, this would amount to saying " I'd like you to look at P because I'd very much like you to look at P " ; and this, in turn, makes no better sense than saying " P is good because it is pleasant ", where " P is good " and " P is pleasant " are held to mean the same. If I am told that I am here using ' good ' descriptively and not evaluatively, I may reply that this is not so : I am using ' good ' evaluatively, at least in any but a restricted, philosopher's use of ' evaluatively '. Also, if the naturalist can appeal to the fact that ' good ' does two kinds of jobs, so can the non-natural-

ist, in order to account for certain uses of ' good ' ; the disagreement would be about which job is primary. My point is that Mr. Hare's analysis, as much as the naturalist's, would keep us from saying what we do say and want to say.

Lincoln College, Oxford.

ON DEFINING DISPOSITION PREDICATES

By JAN BERG

SUPPOSE that we have a disposition D involving the test operation Q and the test reaction R. On the material interpretation of conditional sentences, the simple way of defining ' Dx ' :

$$Dx \equiv .\ Qx \supset Rx,$$

will not do since $\sim Qx \supset (Qx \supset Rx)$, and consequently $\sim Qx \supset Dx$.[1]

There are two directions in which attempts have been made to avoid this difficulty and at the same time keep the construction of conditionals in the material sense. The first way is described by Carnap in Testability and Meaning,[2] where he, as is well-known, replaces explicit definitions by reduction sentences. The second way is described by Eino Kaila[3] and amounts to an explicit definition of disposition terms. According to this method, however, it is necessary to make use of bound property variables :

$$(Df_k)\ Dx \equiv (\exists P)\ (Px\ .\ (\exists y)\ (Py\ .\ Qy)\ .\ (y)\ (Py\ .\ Qy\ .\supset Ry)\ ).$$

This says that x has the dispositional property D if and only if x has a property P such that at least one element of the extension of P has been tested and such that all tested elements have shown the specific reaction.

Against this process Carnap[4] has directed the objection that artificial properties can easily be constructed that lead to incorrect results when admitted as values of the variable ' P '. Of any two objects, a and b, we can form the class of all x such that $x = a$.v. $x = b$. If the property $x = a$.v. $x = b$ is accepted as a value of ' P ',

[1] Cp. L. J. Russell : Communication and Verification, *Proc. Aristotelian Society*, suppl. vol. 13 (1934), pp. 188–90.

[2] *Philosophy of science*, vol. 3 (1936), pp. 440f.

[3] *Den mänskliga kunskapen* (Stockholm 1939), pp. 239–40.

[4] See E. Kaila : Über den physikalischen Realitätsbegriff, *Acta philosophica fennica*, No. 4 (1941), pp. 33–34.

and if $\sim Q(a)$ and $R(b)$ are assumed to be the case, it follows from (Df_k) that both $D(a)$ and $D(b)$. Kaila gives some suggestions as to how these consequences might be avoided by restricting the range of ' P '. This variable is only intended to range over " natural " or " inductive " properties determined in such a way that they " correspond to infinite classes ". Kaila's explanations show that he has in mind a concept substantially identical with what Nelson Goodman[1] has called ' projectible property '. The statement of the meaning of a predicate expressing a projectible property must not require reference to any special object or space-time location. Such a property admits of an inductive projection from known to unknown cases in the sense that if P is a property that occurs with all known elements of a class K, then it is a justified assumption that P also occurs with all hitherto unknown elements of K. Attempts to establish the distinction between projectible and non-projectible properties have turned out, however, to lead into serious difficulties.[2]

But even if this problem of purely universal properties could be solved, a dangerous objection would remain to be averted in the case of (Df_k). Because, as Wedberg[3] has shown, there are properties such that the statement of their meaning does not require a finite extension and such that they lead to erroneous results if admitted as values of ' P ' in (Df_k). The property $Qx \supset Rx$ is of this kind, on the assumption that $(\exists y)\,(Qy \,.\, Ry)$. If we insert ' $Qx \supset Rx$ ' for ' Px ', we again arrive at the consequence that $\sim Qx \supset Dx$.

All this has been recalled only because it applies to a scheme of definition of disposition terms recently proposed by Storer.[4] It will be shown that this attempted solution is closely akin to (Df_k). Storer's method is intended to make the class of, e.g., all soluble things comprise everything that has dissolved when put into water and also everything never put into water but in some property like objects put into water and found to dissolve. In the present terminology (with the time variable ' t ' added), Storer's scheme of definition would look :

$$(Df_s)\quad Dx \equiv .\,(\exists t)\,(Qx, t \,.\, Rx, t) \vee (\exists P)\,(Px \,.\, (\exists y)\,(Py \,.\, (\exists t)\,(Qy, t \,.\, Ry, t)) \,.\, (y)\,(Py \,.\, (\exists t)\,Qy, t \,.\, \supset (\exists t)\,Ry, t)).$$

[1] Cf. his A query on confirmation, *Journal of Philosophy*, vol. 43 (1946).

[2] See N. Goodman : The problem of counterfactual conditionals, part III, *Journal of Philosophy*, vol. 44 (1947), and C. G. Hempel and P. Oppenheim : Studies in the logic of explanation, §6, *Philosophy of Science*, vol. 15 (1948).

[3] A. Wedberg : The logical construction of the world, *Theoria*, vol. 10 (1944), p. 237.

[4] T. Storer : On defining ' soluble ', *Analysis*, vol. 11 (1951).

The relation between (Df_k) and (Df_s) can be realized in this manner. Upon introduction of ' t ' into (Df_k) we get either :

(Df_k') $Dx \equiv (\exists P)\,(Px \,.\, (\exists y)\,(\exists t)\,(Py \,.\, Qy, t) \,.\, (y)\,(t)\,(Py \,.\, Qy, t. \supset Ry, t))$,

or :

(Df_k'') $Dx \equiv (\exists P)\,(Px \,.\, (\exists y)\,(\exists t)\,(Py \,.\, Qy, t) \,.\, (y)\,(Py \,.\, (\exists t)\,Qy, t \,.\, \supset (\exists t)\,Ry, t))$.

The definiens of (Df_s) follows from the definiens of (Df_k'), and the definiens of (Df_k'') follows from the definiens of (Df_s). Moreover, according to the foot-note on p. 137, Storer seems to lay the same restriction upon the range of ' P '.

The general problem of defining disposition predicates may be regarded from two points of view :

(I) Given a theory T containing the predicates ' Q ', ' R ', and logic ; wanted a scheme of definition of the disposition predicate ' (If Q then R) x' holding for *every* T.

(II) Given a *special* theory T containing the predicates ' Q ', ' R ', and logic ; wanted a definition of the disposition predicate ' (If Q then R)x ' holding for T.

Carnap, Kaila, and Storer look for a solution to problem (I).

Concerning problems of kind (II), it might be pointed out that an explicit definition of a particular disposition predicate ' D ' is obtained, within the framework of some special theory T, if there is some property expressible in T such that the major existential quantifier in (Df_k) can be cancelled and ' P ' can be regarded as a constant expressing this property :

$Dx \equiv .\, Px \,.\, (Ey)\,(Py \,.\, Qy) \,.\, (y)\,(Py.Qy. \supset Ry)$.

Such a property must be chosen so that the expression following ' Px ' is true, and consequently that expression can be deleted :

$Dx \equiv Px$.

Certainly, it is not always the case that a disposition consists of only one (not truth-functionally compound) property expressible in T. In many cases, rather, a disposition would be considered to involve a disjunction of properties :

$Dx \equiv .\, P_1x \vee P_2x \vee \ldots \vee P_nx$,

where at least one element of the extension of P_i, for every i, has been tested, and where all tested elements have shown the specific reaction.

Solutions to problem (II) are graded according to the pregnancy of T. In the most pregnant systems, many disposition predicates are readily defined in the way outlined. However,

as we move down the scale to scantier systems, this method of definition grows more and more precarious. It is true that it may sometimes be possible to define disposition terms in systems less pregnant than physics and chemistry by means of other predicates available within the theory, but here the risk is much greater that it will prove necessary to introduce further disjunctive components in the definiens when fresh scientific discoveries extort a revision and expansion of the system. Case (I) may be considered as a borderline case, where a method of definition is sought for maximally poor systems.

There is another approach to problem (I), on which the material interpretation of the conditional is abandoned and the sentential function of the definiens is construed with verbs in the subjunctive mood:

Dx ≡. If Qx were the case, then Rx would be.

A subjunctive hypothetical is usually employed when the antecedent is assumed to be false, and hence it cannot be interpreted as a truth-function. If the exact meaning of such contrary-to-fact conditionals could be determined, we would have an answer to the general problem (I).

A painstaking attempt to interpret contrafactual conditionals has been carried through by Reichenbach.[1] His analysis is based upon the concept of nomological sentence, which is intended to embrace both logical and empirical laws. A sentence is nomological if it follows from an original nomological sentence, and a necessary condition for a sentence to be nomological and original is that it contains only expressions of universal (projectible) properties.

My intention is to show that Reichenbach's analysis, even if it were satisfactory for its own purpose, would be of little significance for the problem of defining disposition predicates.

To the general form:

(C) If . . . x . . . were, then —x— would be,

where the antecedent may involve many different conditions, of course, there belong two main types of contrafactual conditionals.

(i) The first kind consists of those instances of (C) in which the variable 'x' has an *essential* occurrence.[2]

(ii) The second kind comprises those instances of (C) in which the variable 'x' occurs *vacuously*.[2]

[1] H. Reichenbach: *Elements of symbolic logic* (1947), chap. VIII. In the newly-published posthumous work: Nomological statements and admissible operations, *Studies in logic and the foundations of mathematics*, No. 11 (1954), there is a still more penetrating analysis of these questions. The remark that I am going to make will be valid for both systems.

[2] In Quine's sense; vide his *Mathematical logic* (1951), p. 2.

On the interpretation (i) of (C), the universe is divided into two parts, i.e., the class of all things satisfying (C), and the class of all things that do not satisfy (C). Obviously, the contrary-to-fact conditionals needed in the analysis of dispositional concepts is of kind (i).[1]

Hypotheses of kind (ii), on the contrary, are either universal or empty: they hold good of either all objects in the universe or none. It is important to notice that Reichenbach treats only counterfactuals of this kind. He translates such a conditional into a sentence of the metalanguage stating that the general implication from the antecedent to the consequent is nomological. As an example:

> For all x, if x were NaCl, and if x were put into water, then x would dissolve,

would be rendered like this:

'(x) (NaClx . Qx . ⊃ Rx)' is nomological.

Now the question is whether we might use counterfactuals of kind (ii) in the analysis of those of kind (i). In this direction, an obvious way to analyse the true sentence 'If Qx were, then Rx would be,' where 'x' occurs essentially, would be to say that there is a property P such that x has P and such that the sentence 'If Px and Qx were, then Rx would be' is true and has a vacuous occurrence of 'x'. In Reichenbach's systems, accordingly, a predicate 'D' expressing a disposition that involves the test operation Q and the test reaction R would be defined as follows:

Dx ≡ (∃P) (Px . '(y) (Py . Qy . ⊃ Ry)' is nomological).

That this definition will not do is easily seen, since Wedberg's above-mentioned objection to Kaila's formula can, with a slight modification, be applied here also. If 'R' and 'Q' are predicates admitted to appear in nomological sentences, the property Qx ⊃ Rx could be taken as a value of 'P'. Since logical truths are included in the class of nomological sentences, the sentence '(y) (Qy ⊃ Ry . Qy . ⊃ Ry)' is nomological.

This shows that an interpretation of a large class of contrary-to-fact conditionals would not help us to a satisfactory theory of disposition predicates.

University of Stockholm.

[1] In Pap's *Elements of analytic philosophy*, p. 289, there is a confusion connected with this point. The author discusses the definiens A: 'if x is immersed in water, then x dissolves,' and then he "substitutes" 'salt' for 'x' and considers the definiens A′: 'if salt is immersed in water, then it dissolves'. This, however, alters the whole problem. If we rewrite A′ as: 'if x is salt and x is immersed in water, then x dissolves', we see that the variable 'x' has a vacuous occurrence in A′ but occurs essentially in A.

PEOPLE AND PART-WHOLE TALK

By H. HUDSON

MANY philosophers have maintained that it is the mind, or some other-wordly or unwitnessable part or component of the person, which thinks. In more recent times certain physiologists and others have maintained, for reasons which need not concern us here, that it is really the brain that does the work and gets the results when we think. Accordingly, it is really the brain that thinks. But in ordinary conversation we say that the man thinks. I shall try to show that this latter is correct and that the other two versions are incorrect. They involve the illicit use of " part-whole talk ".

People have often protested, and rightly, against the division of a person into two distinct parts or entities ; and indeed there is something absurd about saying that some part of a person is thinking—as if there was some other part of him which is not. And a man's brain is, of course, a component part of the man ; and a man's mind has been regarded, and doubtless still is regarded by many, as his other-wordly and unwitnessable component part. But we should not try to correct these views that the mind thinks or that the brain does, by talking about the uniqueness of wholes ; their transcendence of their parts ; and of their emergent properties and activities ; as some have done. Such ways of talking are themselves mistaken when we are concerned with people thinking, for instance. To say that the whole man thinks is as absurd as to say that some part of him does. What we must do is to try to be clearer about the conditions under which " part-whole talk " is legitimate, and those under which it is not.

I will deal first of all with the view that it is really the brain that thinks, ponders, or deliberates. Suppose we ask a person who believes that it is really the brain that thinks :—" Is the whole of this thing that thinks, thinking now ? " or " Is every part of it thinking ? " We are quite entitled to ask him such questions, for if we asked :— " Does the whole person think ?" or " Does every part of him do it ? " ; his reply would be : " No, only one part—his brain ; it does this kind of thing ".

There is something very absurd in saying that " every part of his brain is thinking " when his brain is thinking. ' Thinking ' is not like ' vibrating ' ; ' trembling ' ; ' quivering ' ; or even

'aching'. We can say that something is vibrating all over, or that every part of it is vibrating; or that not every part of it is vibrating. For want of a better name we could perhaps call terms such as 'vibrating'; 'trembling'; 'quivering'; and 'aching'; Diffusion Predicates. There are many other Diffusion Predicates which are not the names of activities; for example: 'red'; 'hard'; 'hot'; 'wet'; and so on.

'Thinking' is not a Diffusion Predicate; it is not diffused over an area, or alternatively it does not make sense to say that it extends over one. In this respect 'thinking' is much more like 'hanging' than it is like 'vibrating'. We may say: "The whole picture is hanging from the hook on the wall"; but when we say this we do not suggest that every part of the picture is hanging. All that we mean is that the picture in question has no part missing; or that it is "complete". If part of the picture were missing, we would not say that this part is not hanging from the hook on the wall; but merely that it was missing from the picture hanging from the hook on the wall.

When the picture hangs from the hook on the wall, there are not a vast number of hangings which constitute or make up, as it were, the one synthetic hanging. James Doe may hang by the neck until he is dead, but his neck does not hang, neither do his legs nor his head hang. His hands may tremble however, while his head and legs do not. We could ask significantly whether his hands tremble simultaneously or successively; but we could not ask significantly whether all the parts of Doe hang simultaneously or successively; as if, for instance, it were entirely possible that his torso does not hang at the same time as his legs, or that everything hangs together when he is hung. We could, of course, hang various parts of Doe separately; and in this respect 'thinking' is not at all like 'hanging', for the different parts of Doe cannot think separately.

'Thinking' resembles 'hanging' however in the way in which it differs from 'trembling' or 'vibrating'. It belongs to a different category. If we said that every part of his brain was thinking, or that some sub-part of his brain was, then we would have as many thinking things, each thinking simultaneously, as there are parts or sub-parts of the thing or sub-part in question. Yet, if we deny the sense of "every part is thinking" or "some part is thinking" or "not every part is thinking", what sense has: "the whole brain is thinking" or "the entire brain is thinking"?

If we regard expressions such as:—'all'; 'all of'; 'the

whole of' ; ' every part of' ; ' none of' ; ' some of' ; ' some part of' ; ' not every part of' ; when none of these have the meaning of ' completeness ' or ' incompleteness ', as ' quantifiers ' ; then we could say that sentences in which activities such as 'hanging' ; 'sitting' ; 'standing' ; 'playing' ; and 'thinking' are attributed to some individual ; cannot be so quantified. They are strictly ' individual sentences '.

When we use the expression : " the engine is vibrating ", this is less strong than the expression : " the whole engine is vibrating " ; and to say " he is trembling " is less strong than to say : " he is trembling all over ". To say that the engine is vibrating is quite compatible with pointing out or admitting that one or two parts of it are not. Moreover, we do not usually mean that every single bit of the engine is vibrating when we say that the engine is vibrating. Similarly with when we say that a man is trembling. But to say that a picture is hanging from a hook on the wall is certainly not compatible with saying that possibly part of it is not hanging, when the entire picture is present and correct. Similarly, with when we say that a man is thinking.

Since vibrating and trembling spread or extend over an area while hanging and thinking do not, some people may feel puzzled and ask : " How can something such as a picture which extends over an area, nevertheless be related to something else such as a hook and a wall which extend over an area, in a way which it is nonsensical to characterize as extended ? " But there is no puzzle here ; for this is just what makes the relationship possible. If the hanging or thinking were said to be ' unextended ' or ' extensionless ', then indeed we would have every reason to be puzzled. But if it is nonsense to say that they are ' extended ', or can occur, or be spread out, over an area, then to say that they are ' extensionless ' is equally nonsensical. This can be applied to the relationship between the man ; his brain ; and what he is thinking about ; when he is thinking.

We will now suppose that the person who says that the brain thinks, agrees about the absurdity of saying that every part of a thinking thing is thinking ; that some part is or is not doing it ; or that the whole thing is doing it ; and that he also agrees that it is just the thing, the individual—in this case, the individual organ, the brain, which thinks.

Now the individual organ or part which thinks is nevertheless part of a distinct individual man. But if it is illegitimate to speak of a part of the individual brain which thinks, is it legiti-

mate to speak about part of the individual man which thinks and of part of him which does not ? If it is illegitimate to speak of the whole or of parts of a thing in connection with an activity such as thinking, then the answer to the question is " No ". When we speak of thinking in connection with Doe or with his brain, to regard his brain as that part of him which does the thinking is senseless ; and it makes nonsense to distinguish in part-whole idioms between Doe and his brain in so far as we say that Doe is thinking.

Nor will the position be any easier, if we say that some spiritual or other-wordly part or organ of the person does the thinking. It is just as foolish to deny that any or every extended part of the person thinks, as it is to affirm that any extended part of him thinks ; and to say that an other-wordly or spiritual part of him thinks, involves a denial that any mundane, material, or witnessable part of him does.

The sub-division of the human being into two component parts, one of which is a special organ that carries out activities such as ' thinking ' ; ' pondering ' ; and ' deliberating ' ; depends upon a category mistake. It is to treat such activities as if they were like ' trembling ' and ' vibrating ' ; or it is to muddle the activities of individuals with respect to which " part-whole talk " is quite appropriate with those with respect to which it is quite illegitimate. Consequently to say that the man thinks is correct, but to say that his mind or brain does is incorrect.

Victoria University College, New Zealand.

THE GAMBLER'S FALLACY—A FURTHER NOTE

By JOHN C. SIMOPOULOS

THERE is, in the nature of the case, so much ground for supposing there to be a strong taint of hope in the opinions of the gambler that it would seem more in point to kill such misguided hope by exhibiting clearly the spurious pedigree of the opinions which support it than to suggest that they may, after all, be right.

The main contention in Mr. Atkinson's note[1] appears to be that the gambler's fallacy cannot "be disallowed on *a priori* grounds". He clearly implies, however, that this could be done if it were made "part of the definition of a game of chance that there can be no 'system', such as betting on tails after two heads, for improving one's chances of winning." But "improving one's chances of winning" needs to be clarified. Suppose I have been trying to win by guessing Heads or Tails of the throws of a coin. "Tails," I say; and it's Heads. I have lost a penny. "Tails", I say again; and it's Heads again. This time I stake and lose twopence. I am threepence down. And so on. After the nth consecutive Head and my nth consecutive wrong guess I am 2^n-1 pence down. I may now stop. Then I have lost 2^n-1 pence. Or I may go on. If I go on I can lay down any sum up to and including 2^n-1 pence. And if I do this I do not win; the best that could happen to me would be not losing. Or I can lay down any sum greater than 2^n-1 pence. This means that I *may* be up on the day's gambling. Had I lost my head earlier on I might have laid out so much in the firm expectation of Tails that at the end of the *n*th failure I would have had insufficient resources to do any more than, at the best, recover *some* of my losses; and I might even have had nothing at all. Quite obviously, then, doubling your stakes has something to recommend it as a gambling system.

Now the point I want to make about the system of doubling your stakes is that what it has to do with probability is not as it at first appears. It is not necessary to connect it with the absurd view that the longer the run of Heads the more certain it is that the next throw will be Tails. This, the maturity fallacy, may well owe its existence (*a*) to ignoring the difference between

[1] *Analysis*, Vol. 14, p. 149.

the case where the number of α events and the number of β events are both finite (white and black balls in a bag) and the case where there is "no bag" (coin-throws); and (*b*) to an asymmetrical comparison: HHHH is exactly as rare or common as HHHT if you regard them just as HHHH and HHHT respectively; it is, however, exactly four times as rare as HHHT if you regard them as four Heads and three Heads-and-a Tail respectively. Now suppose you have had your three Heads; what is wrong with saying to yourself, in the way in which a gambler might perhaps try to rationalize his behaviour, that as a show of four Heads is only a quarter as likely as a show of three Heads-and-a Tail there is far more likelihood of Tails coming up this time? What is wrong in this *reasoning*, I think, is that information is being ignored in the HHHT case. For after HHH the only possibility of three H's and a T occurring is precisely HHHT, which is on a par with HHHH. That is to say *before* the quartet of throws a comparison of the probabilities of four Heads and three Heads-and-a Tail is in point; but after the third consecutive Head a comparison is only in point of HHHH with HHHT, and this comparison is the same as that of H with T.

So much, then, for a possible etiology of the maturity fallacy. The system of doubling your stakes does not depend on this fallacy. For it "works" whatever choice you make. That is to say if after my n unsuccessful guesses I am determined to give myself the chance of going away a penny richer than I came it does not matter which possibility I stake my 2^{n-1} pennies on (where the nth gamble is in question), Heads or Tails. The reason I am at pains to make this clear is to show that a gambling system can be intelligently (if somewhat rashly) conceived and put into operation without any probability-forecast being made involving a fallacy. The sort of system which if it *could* be worked —that is to say which might reasonably be relied on—would mean that what we were playing at was not a game of chance is not the sort I have just described (doubling your stakes) but one explicitly based on worked-out forecasts. And it is worked-out forecasts which in a genuine game of chance have the lowly status of wishful thinking. Doubling your stakes is somewhat more respectable: thoughtful wishing. Now on the assumption that Mr. Kneale, in the passage referred to by Mr. Atkinson, is talking of systems purporting to be based on worked-out forecasts, not only is he quite obviously right but it does not, as

Mr. Atkinson says it does, follow from what Mr. Kneale says that "it is a mistake to call roulette, coin tossing, dice rolling, etc., games of chance." For Mr. Atkinson's saying that it does rests on an unproved and unargued assumption that there can be or is a gambler's system. And I am suggesting that there are two kinds of such system one fallacious the other not, and that it is only if, *per impossible*, the fallacious system were, as a matter of fact, not fallacious what it applied to ought not to be called a game of chance.

Lastly, as the gambler's or maturity fallacy is not some observational shortcoming or eccentricity but a general statement based on arguments it will not do to try and deny its wrongness by ignoring its structure. The gambler's fallacy is not a false statement about a matter of fact but a bit of spurious probability theory; and as such it must be judged.

St. Catherine's Society,
Oxford.